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Note on the Determination of the Longitude Paris-Greenwich in the Year 1902.

(*Communicated by the Astronomer-Royal.*)

In view of the discordance between previous determinations of the longitude of Paris-Greenwich, the International Geodetic Conference in 1898 expressed the view that a redetermination was desirable. It was arranged by M. Lœwy and the Astronomer-Royal that the redetermination should be undertaken in concert by the two observatories of Paris and Greenwich. Owing to the determination of the longitude Greenwich-Killorglin in 1898 and the eclipse expeditions of 1900 and 1901 it was not found practicable to commence this work till 1902.

The programme arranged in conjunction with M. Lœwy provided for the determination being made in two stages—in the spring and autumn of 1902. The instruments used were the four portable reversible transits belonging to the Royal Observatory which had been used in previous longitude determinations from 1888 to 1898. Each observer kept the same instrument throughout, taking it with him from Paris to Greenwich, or Greenwich to Paris. The instruments were distributed thus :—

Observer...	M. Bigourdan	M. Lancelin	Mr. Dyson	Mr. Hollis
Transit instrument	E	D	B	C

The observing stations at Greenwich were the Transit Pavilion and an adjacent wooden observing hut in the front court; the observing stations at Paris were in the grounds to the south of the Observatory and a little to the east of Cassini's meridian.

The French observers originally selected by M. Lœwy were M. Renan and M. Bigourdan, but owing to the illness of M. Renan he was replaced by M. Lancelin. The English observers were Mr. Dyson and Mr. Hollis.

Independent determinations of longitude were thus made simultaneously by the French and English observers, using adjacent stations and similar instruments. A double interchange of observers and instruments was made both in the spring and autumn. In the autumn the stands were also changed with the instruments. The programme of observations required clock-errors to be determined simultaneously at Greenwich and Paris on three, six, and three full nights respectively in the three parts of each determination. The following table gives details of the number of nights of observation of the English observers :—

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Date.	Observer at		Signals Exchanged.	Number of Nights on which Clock-error was Determined			Weight.
	Paris.	Green-wich.		Paris.	Green-wich.	Simul-taneously.	
1902.							
I. Mar. 17–30	Dyson	Hollis	13 nights	5	9	4	7
II. Apr. 6–24	Hollis	Dyson	19 „	10	11	6	12
III. Apr. 23–May 3	Dyson	Hollis	7 „	3	5	3	6
I. Sept. 21–26	Hollis	Dyson	6 „	4	6	4	7
II. Sept. 29–Oct. 22	Dyson	Hollis	24 „	11	13	8	14
III. Oct. 25–Nov. 4	Hollis	Dyson	11 „	8	8	5	8

The complete programme for a full night's observations was as follows :—

I. Observation of mark and nadir.
II. *Micrometer E.** : Eight time stars, one or more polar stars, two or three observations of level.
III. *Micrometer W.* : Eight time stars, one or more polar stars, two or three observations of level.
IV. Observation of nadir.
V. *Micrometer W.* : Eight time stars, one or more polar stars, two or three observations of level.
VI. *Micrometer E.* : Eight time stars, one or more polar stars, two or three observations of level.
VII. Observation of nadir and mark.

Two interchanges of telegraphic signals were made in the course of the evening, signals being sent in each case from Greenwich to Paris, and Paris to Greenwich, and registered on the chronographs both at the sending and receiving stations.

The Collimation Error.—This error was eliminated by the reversal of the instrument. Its constancy, however, as determined in four different ways, viz. directly by reversal on the mark and on the nadir, and indirectly by the agreement of the polar stars observed micrometer East and micrometer West and of the time stars observed micrometer East and micrometer West, gave the most satisfactory proof of the stability of the instrument and fixity of the object glass.

The Level Error was determined both by nadir observations and by the striding level, using the micrometer screw of the level so as to eliminate as far as possible any variation in scale value. As results for the deduced longitude as derived from nadirs and levels separately differ by only $^{s}\cdot 013$ for the spring determination and $^{s}\cdot 002$ for that in the autumn, it is clear that no appreciable error in the longitude is attributable to this cause.

The Azimuth Error.—The value of n adopted for each night

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* The two positions of the instrument are denoted "Micrometer E." and "Micrometer W."

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usually depended on the mean of two or three observations with the micrometer East, and an equal number with the micrometer West. The effect on the clock error is n (tan δ—tan λ), where δ is the mean declination of the time stars and λ the latitude. Stars of high declination (but always south of the zenith) were used, so that the above factor is small. The accordance of the separate determinations of n shows that no appreciable error, systematic or accidental, can be attributed to this cause.

Clock-error.—The clocks used were the standard sidereal clocks of the two observatories, Greenwich and Paris. The rates were very uniform. The catalogue of stars used for determination of clock-error was prepared by M. Lœwy, the right ascensions being reduced to Newcomb's fundamental system. At the conclusion of the longitude determination corrections were deduced to the adopted positions of the stars from the combined observations of the French and English observers. These corrections were applied, but did not alter the resulting longitude by ^{s}·001. Incidentally the deduction of these corrections showed that the probable accidental error of the observation of a star transit was ±^{s}·033.

The Telegraphic Signals.—At each station the signals sent and received were recorded by means of the same relay and local circuit as the observations of transits. By the insertion of suitable resistance in a parallel circuit the current actuating the relay was made the same when recording the observations of transits and of signals received or sent. The time of transmission in one direction was found to be 0^{s}·021 from the mean of the spring determinations and 0^{s}·022 in the autumn. The differences from these means on individual nights were usually only a few thousandths of a second.

The following table exhibits the separate determinations of the difference of longitude between Cassini's meridian at Paris and that of the transit circle at Greenwich. The difference of the personal equation D – H was found to be -0^{s}·0421 in the spring and -0^{s}·0425 in the autumn. The value —^{s}·042 has been adopted throughout and applied to derive column 3 from column 2.

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Difference of Longitude between Paris and Greenwich.—Spring Determination.

	1902.	Determined Difference of Longitude.	Corrected for Personal Equation.	Weight.	Discordance from Mean $9^m\ 20^s{\cdot}977$.	Discordance from General Mean $9^m\ 20^s{\cdot}932$.
		m s	m s		s	
Dyson at Paris Hollis at Greenwich	Mar. 19	9 21·060	9 21·018	2	+0·041	+0·086
	23	·108	·066	1	+0·089	+0·134
	25	·054	·012	2	+0·035	+0·080
	28	·096	·054	2	+0·077	+0·122
Hollis at Paris Dyson at Greenwich	Apr. 13	9 20·979	9 21·021	2	+0·044	+0·089
	17	·966	·008	2	+0·031	+0·076
	18	·901	20·943	2	−0·034	+0·011
	20	·940	·982	2	+0·005	+0·050
	23	·922	·964	2	−0·013	+0·032
	24	·899	·941	2	−0·036	+0·009
Dyson at Paris Hollis at Greenwich	Apr. 28	9 20·901	9 20·859	2	−0·118	−0·073
	May 1	·979	·937	2	−0·040	+0·005
	3	·977	·935	2	−0·042	+0·003
Mean discordance for Weight 2			...	...	±0·047	±0·059

Autumn Determination

	1902.	Determined Difference of Longitude.	Corrected for Personal Equation.	Weight.	Discordance from Mean $9^m\ 20^s{\cdot}910$.	Discordance from General Mean $9^m\ 20^s{\cdot}932$.
		m s	m s		s	
Hollis at Paris Dyson at Greenwich	Sept. 21	9 20·868	9 20·910	1	0·000	−0·022
	22	·808	·850	2	−0·060	−0·082
	24	·838	·880	2	−0·030	−0·052
	26	·846	·888	2	−0·022	−0·044
Dyson at Paris Hollis at Greenwich	Oct. 8	9 20·926	9 20·884	1	−0·026	−0·048
	9	·969	·927	1	+0·017	−0·005
	10	·935	·893	2	−0·017	−0·039
	14	·967	·925	2	+0·015	−0·007
	16	·954	·912	2	+0·002	−0·020
	17	·949	·907	2	−0·003	−0·025
	21	·950	·908	2	−0·002	−0·024
	22	·965	·923	2	+0·013	−0·009
Hollis at Paris Dyson at Greenwich	Oct. 27	9 20·919	9 20·961	1	+0·051	+0·029
	29	·874	·916	2	+0·006	−0·016
	Nov. 2	·874	·916	2	+0·006	−0·016
	3	·873	·915	1	+0·005	−0·017
	4	·936	·978	2	+0·068	+0·046
Mean discordance for Weight 2			...	...	±0·019	±0·028

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If the determinations in the spring and autumn are considered separately we find—

(Spring) Long. = $9^{\mathrm{m}}\ 20^{\mathrm{s}}{\cdot}977 \pm {}^{\mathrm{s}}{\cdot}0110$. Prob. error of full night = $\pm {}^{\mathrm{s}}{\cdot}039$

(Autumn) Long. = $9\ 20{\cdot}910 \pm {\cdot}0042$. Prob. error of full night = $\pm {\cdot}016$

The smaller probable error in the second series entitles it to greater weight, but the difference between the two determinations being so much larger than the probable errors would indicate, the two results were not combined with the relative weights given above. If the simple mean is taken $9^{\mathrm{m}}\ 20^{\mathrm{s}}{\cdot}943$ and the discordances formed from it, the probable error of a full night (weight 2) in the spring determination is found to be $\pm^{\mathrm{s}}{\cdot}051$, and in the autumn determination $\pm^{\mathrm{s}}{\cdot}037$. The autumn determination has therefore been given a double weight as compared with that in the spring, and the resulting value of the longitude is

$$9^{\mathrm{m}}\ 20^{\mathrm{s}}{\cdot}932 \pm {}^{\mathrm{s}}{\cdot}0060.$$

The discordances from this value are given in the last column.

The systematic difference between Parts I. and III. in the autumn determination is much smaller than in the spring. This is an additional reason for giving a double weight to the latter determination.

It does not seem possible that the systematic discordances shown in the separate parts of the two determinations can be attributed to instrumental errors. Variation of the personal equations of the observers is a natural explanation, and in support of this it may be noticed that the largest discordances are in Series I. of each of the determinations, especially of the one made in the spring. The observers' personal equations had possibly not settled down to the values they subsequently acquired. The advantage of the double exchange of observers and of a determination in the spring and the autumn is its effect on the elimination from the result of such systematic errors.

The value found by the French observers, MM. Bigourdan and Lancelin, is given in the *Comptes Rendus*, vol. cxxxix. p. 1014, as $9^{\mathrm{m}}\ 20^{\mathrm{s}}{\cdot}974 \pm {}^{\mathrm{s}}{\cdot}008$.

The results for the several determinations of the Paris-Greenwich longitude by the English observers are :—

	Longitude.	Prob. Error.
	m s	s
1888	9 20·85	± ·018
1892	9 20·79	± ·022
1902 I.	9 20·977	± ·011
1902 II.	9 20·910	± ·004

Royal Observatory, Greenwich:
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On the Temperature of Sun-spots and the Spectrum of an artificial one. By W. E. Wilson, D.Sc., F.R.S.

The most usual theory of the cause of the darkness of sun-spots is that formulated by De la Rue, Stewart and others, viz. that spots are produced by the down-rush of cooler material on to the photosphere; and the fact most strongly insisted on in support of this theory is that in the spectrum of a spot there are a considerable number of the Fraunhofer lines, both darkened and widened.

The photograph reproduced on Plate 8 of what I have called the "Spectrum of an Artificial Sun-spot," as I believe it will throw a good deal of doubt on the validity of the usual sun-spot theory, and may possibly go towards proving that in sun-spots we have regions of a higher temperature than the surrounding photosphere, and not a cooler one.

The photograph was obtained by the following experiment. An opal globe surrounding an electric arc represented the Sun's photosphere. A small patch of thin paper on the globe, which reduced its light, say, 50 per cent., was my sun-spot. A rectangular glass cell containing nitric oxide fumes was my chromosphere or absorbing layer. A lens was arranged to throw an image of the paper patch on part of the slit of the spectroscope in the same way as a real sun-spot. The photograph of the spectrum shows that where the dark absorption lines cross the band of general absorption due to the paper patch a good many of the lines are both darkened and widened. I find that all lines which have nebulous edges are widened, and lines that have sharp edges, or the sharp edges of bands are not affected. In fact, in this photograph we have an almost similar effect to that of a real sun-spot without having to call in anything like a cool down-rush, but merely by reducing the radiation from the background.

Now what are the causes which give us a dark-line absorption spectrum? In the first place the absorbing gas must be at a lower temperature, but also it must be thick enough to be opaque to those particular wave-lengths which give the positions to its dark lines. As we increase the thickness of the absorbing gas we first get the darkest lines, and then more and more of the lines appear, until ultimately the entire continuous spectrum would be blotted out. This shows that for certain thicknesses of the absorbing gas it is quite transparent to some wave-lengths, but to which ultimately it becomes opaque; and this takes place by no alteration in the temperature. If we have a feeble source of light giving the continuous spectrum, the absorbing layer becomes opaque with a less depth than when the source is of greater brilliancy.

The absorption spectrum of water vapour in the Earth's